

BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater–Tidal Marshes

Charles Simenstad

Final Selection Panel Review

Proposal Title

#0246: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater-Tidal Marshes

Funding:

Fund with future funds

Amount: \$1,500,000

The public comments received were fundamentally an explanation of how the research team would modify the project with the significant budget reduction proposed by the Final Selection Panel. The research team clearly heard the message being sent in the several reviews and modified the proposal accordingly. They should be commended for that. The modifications they have made are well conceived and clearly explained. The panel recognizes that the contributions of the scaled down research will be less than promised with the original proposal, but the research team has clearly identified what would be accomplished with this funding, and it would make a significant contribution to the CALFED restoration program.

The final Selection Panel agreed with its original recommendation on the merits of this proposal. Due to the recent reduction in funds available for the Science Program's 2004 PSP, the Selection Panel has been forced to place this proposal in the Fund with Future Funds category. This decision was based solely on the current programmatic priorities of CALFED and the current level of available funds for purposes of supporting research efforts of this nature. This decision was not a reflection of the technical merit of this proposal.

#0246: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evol...

Public Comments

The following public comments were received for this proposal.

Proposal # 246



17 June 2005

Mr. Ladd Lougee
Research Coordinator
California Bay-Delta Authority
CALFED Science Program
650 Capitol Mall, 5th Floor
Sacramento, CA 95814

Subject: Response on funding recommendations for CALFED Bay-Delta PSP #0246

Dear Ladd:

As the lead Principal Investigator (PI) of the proposed proposal, [#0246: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater-Tidal Marshes](#), to the Bay-Delta Authority (CALFED) Science Program PSP currently under consideration, I wish to respond on the part of our (BREACH III) research team to the reviewer and panel recommendations and describe how we would propose to meet the required change in scope of the BREACH III research project.

Based on our experience in the BREACH I and BREACH II research projects, as well as interdisciplinary, ecosystem scale research that many of us have been involved in, we believe that we designed an intensive, process-based project that effectively integrated physical and ecological sciences to test and potentially resolve some critical questions about restoration in the north Delta. The reviews almost completely supported that premise, but pointed out several scientific uncertainties (see below) that appear to be the source of the ~35% funding reduction. Although we can't help but echo Technical Reviewer #2, that they were "gratified to see a request for funds that seem adequate to support a truly integrated and comprehensive analysis of tidal wetland restoration/evolution, trophic structure and energy flow," we recognize the extensive funding constraints on the CALFED Science Program and certainly appreciate the Selection Panel's conclusion that substantial scientific contributions could be realized by CALFED even with the substantial funding reduction. Accordingly, we have re-examined our research study plan and in the light of the reviewers' and Selection Panel's recommendations have scaled down the research accordingly, but with acknowledgement of the consequences to our originally anticipated results and contributions to the CALFED restoration program.

The objectives of this response are to: (1) briefly comment on reviewer points, where explanation may clarify our revised research plan; (2) describe how reduced funding could be implemented by reductions in scope, objectives, and tasks; (3) delineate the

consequences of these changes will have in the scope of the revised research plan and (4) provide proposal figures that could not reach the reviewers through the normal CALFED PSP submission process.

(1) Responses to reviewers and Selection Panel Recommendations

We thoroughly appreciate both detailed review comments and insightful suggestions provided by the three reviewers. All three obviously invested considerable time evaluating the proposal and thinking about our approach, and their comments were broadly complimentary and always constructive. These responses are intended only to provide very brief clarification that reflects on our revised approach to a reduced research plan.

- More mechanistic understanding of plant colonization process--distances between established vegetation meadows and newly emergent patches should be evaluated, and both pollen limitation and vegetative expansion (Technical Reviewer #1): We originally and still intend that the distance between established vegetation and newly emergent patches will be included as a component of the study. Although pollen limitation would be an interesting aspect of this system to evaluate, the primary question being addressed by this research is what the physical establishment thresholds are once plant propagules are dispersed to the site. Dispersal, physiological tolerance, and biotic interactions are recognized as the three processes that determine a species abundance at any given site; our research assumes dispersal (since colonization by tules is already occurring) and is focused on elucidating the relative importance of physiological tolerance and biotic interactions on establishment and expansion. Due to budgetary constraints, it will not be possible to include an evaluation of pollen limitation. Edaphic factors will be monitored (but not manipulated) at all transplant and natural colonization stands in the study. This will provide a range of edaphic and hydrologic conditions that will be analyzed in terms of vegetative establishment and expansion of the key species. Due to limited fiscal capacity, our focus must remain on manipulating only biotic controls (plant species interactions, i.e., competition) on plant species establishment and vegetative spread, and then evaluating the role of hydrologic and edaphic conditions on these plant response characters through correlative observations.
- Minor problem with the evaluation of fish food web dynamics, where it would be “fruitful to select species that are ubiquitous across habitats, and determine whether foraging behavior and trophic status shift upon plant colonization” (Technical Reviewer #1): Based both our BREACH I and BREACH II and IRWM experience, this is absolutely the approach we intend to take.
- The comment that “the hypothesis (H_3) that increased vegetative structure influences the structure, abundance, and behavior of animal communities is trite” (Technical Reviewer #3) legitimately points out the actually intended point of that hypothesis, that assemblage structure and behavior of animals varies across different stages of marsh development. The reviewer stated our point much more succinctly than we did.

- The criticism that our proposed models “are being refined and evaluated based on studies of ONE island suggests that they may not be very good at predicting results on OTHER islands” (Technical Reviewer #3) cannot really be argued in an ideal world; however, the universe of very recently restoring wetlands in the northern Delta from which we can draw is EXCEEDINGLY limited. We chose instead to utilize Liberty Island which, because of its size, represents a system with an exceptionally wide range of vegetation colonization states within one domain. Obviously, in strictly statistical sense, the inferential power of this approach is limited, but from the standpoint of an early transitional and broad space-for-time continuum, Liberty Island represents an excellent opportunity to examine a natural experiment with many applications to restoration in the northern Delta.
- The observation that it will be “difficult to say much for certain about the ecological/behavioral patterns that produce these (food web dynamics) results” because, for instance, “two delta smelt may have the same isotopic signature even though they arrive at those integrative isotopic ratios through different migratory pathways” is insightful but we still hope to be able to distinguish pathways, with the incorporation of $\delta^{34}\text{S}$ in the isotope array (to detect fish that have tapped into brackish food web pathways, to compare the multiple isotopic signature (space) to actual prey composition, and to explore the results of otolith microchemistry among fish of different life history stages within different emerging habitats on Liberty Island.
- Deterministic nature of phenomena is likely to decrease from vegetative succession to fish assemblages to avian assemblages due to greater stochasticity (Technical Reviewer #3): This reviewer’s comment about fish, (invertebrate and) avifauna variability is certainly valid, although not entirely germane to our approach to them. While the “nekton and avian assemblage and ecological patterns are very likely to reflect specific conditions on Liberty Island in the years (two) that these populations are studied,” we do not seek to document a statistically significant numerical response as much as compare the functional (i.e., behavioral, trophic) responses of the biota that lead to or relate to species of concern (particularly fishes of special concern). Furthermore, the proposal was designed to address CALFED Bay-Delta Authority program objectives where response by higher level consumer organisms (fish, avifauna) and intermediary taxa (macroinvertebrate prey) of particular concern to CALFED restoration is essential to closure of the loop between wetland restoration and benefit to species of concern.
- We also point out the additional advantage of assessing restoring habitat use associated with Yolo Bypass that is of particular application to juvenile Chinook salmon as species at risk. Liberty Island is the largest tidal restoration site in the Delta and provides a unique opportunity to examine the response of these fish as they enter restoring wetlands at the transition to the northern Delta. How they, and potentially other species of concern (e.g., Delta smelt) respond to the broad, and rapidly changing Liberty Island landscape has direct implications for

restoration actions and issues over a much broader tidal freshwater scale across the Delta.

(2) Changes to Proposed Research Plan

We have seriously considered how we balance the scope of recommended reduction in funding with our intent to link the processes controlling rates and patterns of vegetation colonization with responses by biota of special concern. To meet this challenge, we have approached it from the perspective of “What can be retained that would still provide the critical information to link those processes?” rather than to completely eliminate specific tasks. The following encapsulate how we would make adjustments with the least damage to the desired scientific information (see below):

- broad scope of reduction in task budgets, with targeted reduction in Tasks 5, 6, 7 and 8, in accordance with reviewers and selection panel recommendation;
- retain critical elements of Tasks 5-7 concentrated on fish, their prey, competitors and predators that are species of concern or diagnostic of the different restoring habitat changes; accordingly, we would reduce or eliminate organisms not tightly linked to evolving habitat (e.g., zooplankton);
- temporal scope of physical and vegetative processes tasks field investigations remain ~2 yr but Task 5-7 would be concentrated (with increased effort over sampling period) within ~1 yr, nested in the total 2-yr field period; the number of study sites, however, would need to be reduced by one, however;
- the temporal distribution of the reduced sampling effort would focus on contrasting seasons (wet vs. dry), while the physical and vegetation measurements would continue through 2 yr (e.g., two growth periods) as originally proposed;
- we would eliminate for all tasks the proposed response to flooding and associated events; where capturing those effects would be entirely opportunistic;
- reduce hydrodynamic model runs from 3 to 2 scenarios; and,
- reduce comprehensive coordination among all agencies and research investigations in Liberty Island region, potentially compensated by increasing (*ad hoc*) coordination and collaboration with CF&G, Solano Land Trust and other science/monitoring activities in the region.

Our budget reallocations to meet these adjustments would involve approximately 30% (Task 6) to 66% (Task 5) reductions among the four targeted tasks (Task 5-8), approximately 15% in the other science tasks, and 15%-30% reduction in the synthesis (least), logistics and coordination (greatest).

(3) Consequences of Changes in Scope of Revised Research Plan

Obviously, we regret having to eliminate or reduce these components. However, we feel that it is critical to recognize the limitations that these steps would impose on the interpretability and benefits of our results:

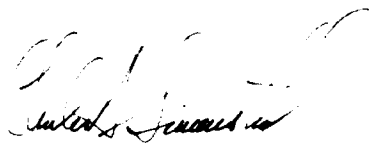
- A more species (of concern) specific attention on the faunal response to habitat landscape shifts will result in a more focused, rather than community ecology level, characterization of the restoration of an emergent wetland landscape mosaic; we still believe that this will have broad applicability to other restoration in the northern Delta, but will not necessarily concerns about other community components such as other fishes and birds;
- reduced frequency and duration of field work on fauna, and commensurate reduction in logistic support, diminishes likelihood of identifying strength of habitat linkages with many of the responding organisms;
- unless it happens to occur within the period of our reduced field investigations, specific elimination of the element to focus on the ecological responses to dramatic, event-driven changes in physical processes will reduce our chances to capture the role of flooding and other events, and linkages to adjacent seasonal floodplain systems such as Yolo Bypass;
- reducing hydrodynamic model runs (by 1/3), will reduce the number of scenarios that can be predicted, specifically excluding large events such as Yolo Bypass flooding);
- reduced coordination, particularly with other related projects (COYOTE), diminishes synthesis potential and scientific efficiency; and,
- reduced funding will require loss of some graduate (and undergraduate) student support.

(4) Proposal Figures

Due to complications and difficulties with the CALFED PSP proposal submission process, the figures accompanying the BREACH III proposal apparently were not available to the technical reviewers. We regret that problem because the figures may have resolved some of the reviewers' questions. In combination with this response, perhaps the Selection Panel will obtain a clearer view of the proposal's technical strengths.

If there is any need or desire for further clarification, we are at your disposal. Thank you for considering this submission.

Yours sincerely,



Prof. Charles Simenstad, Research Associate Professor, University of Washington (Lead PI) on behalf of the BREACH III team

Figure 1 Northern Delta study region; see Figures 2 and 5 for expanded views.

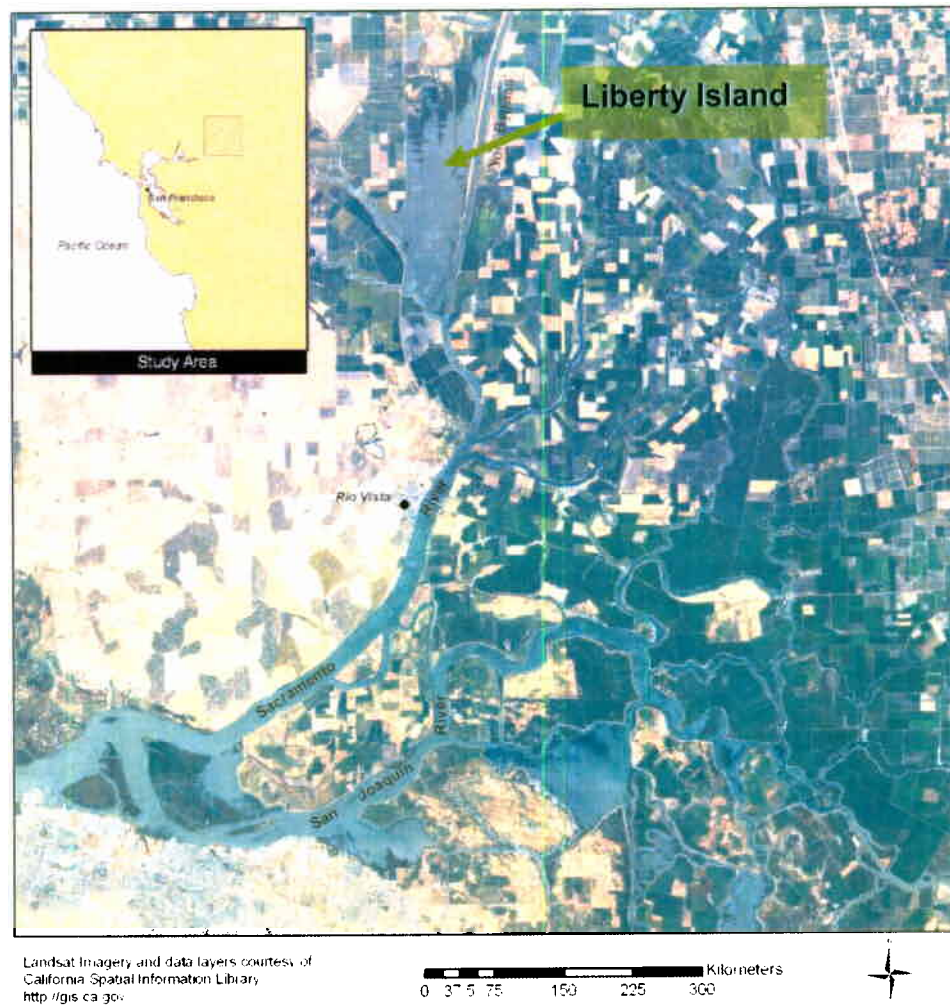
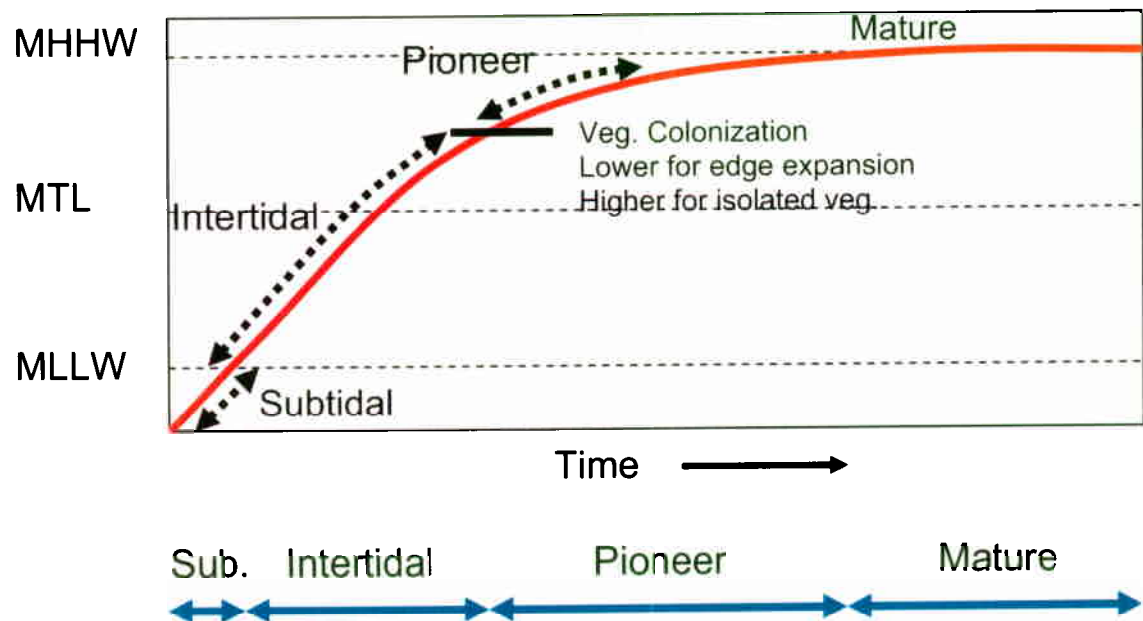


Figure 2 Liberty unflooded and flooded, with photograph insert of new vegetation clones.



Figure 3 BREACH Conceptual Models.

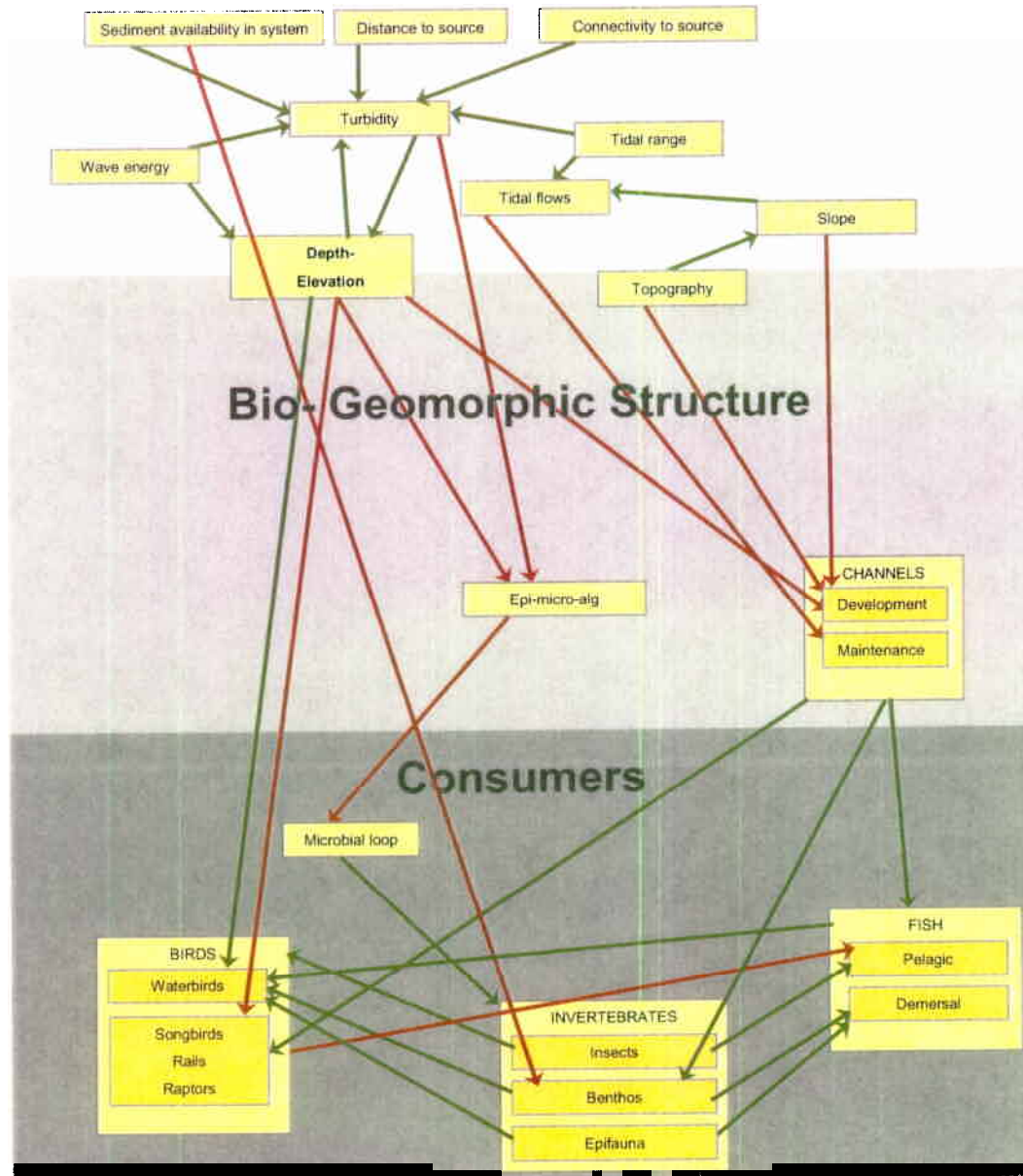
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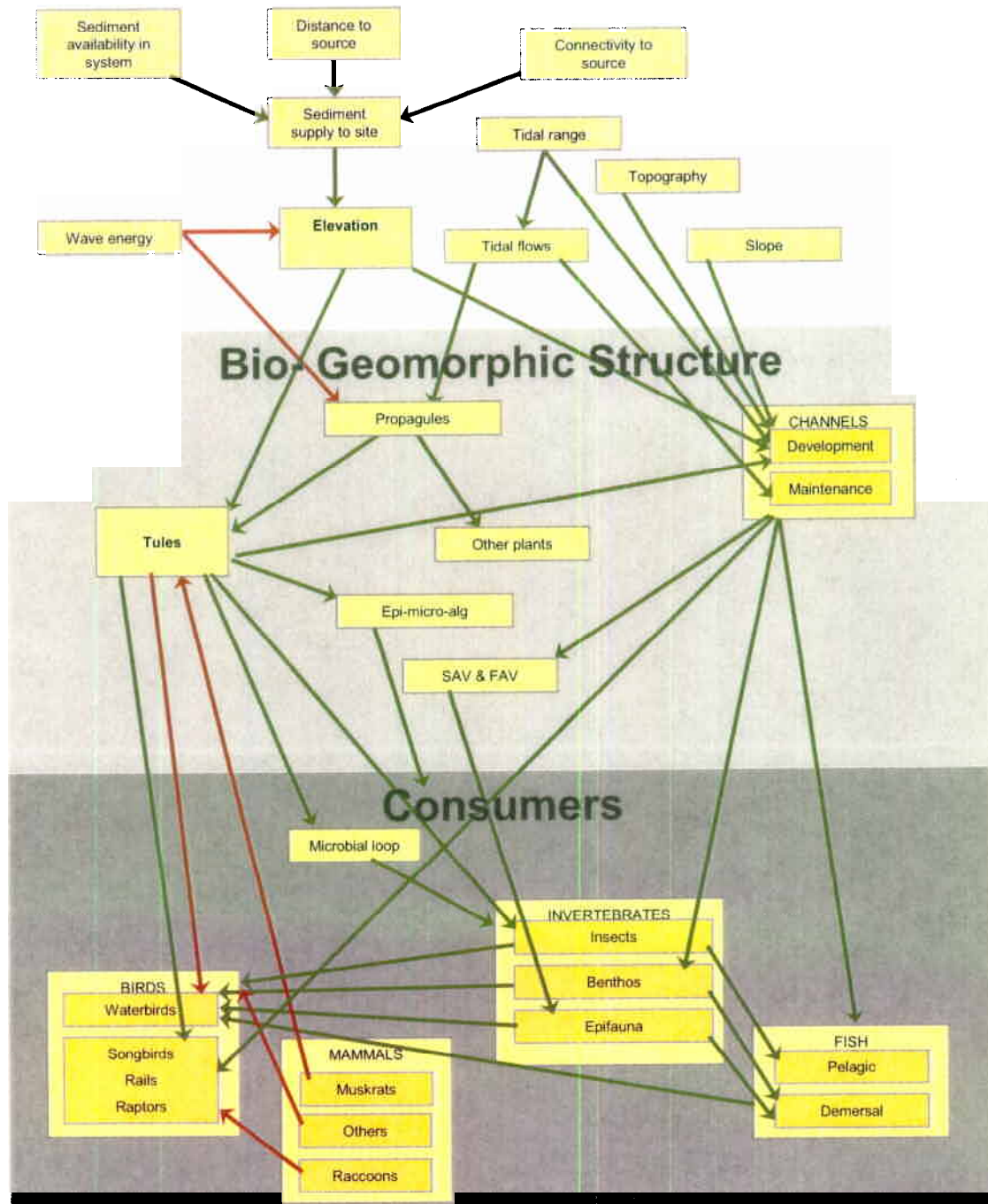
b. BREACH Conceptual Models. Green arrows represent a positive relationship, red arrow represent a negative relationship.

INTERTIDAL

Physical Drivers



PIONEER TULE



d. BREACH Conceptual Models. Green arrows represent a positive relationship, red arrow represent a negative relationship.

MATURE TULE

Physical Drivers

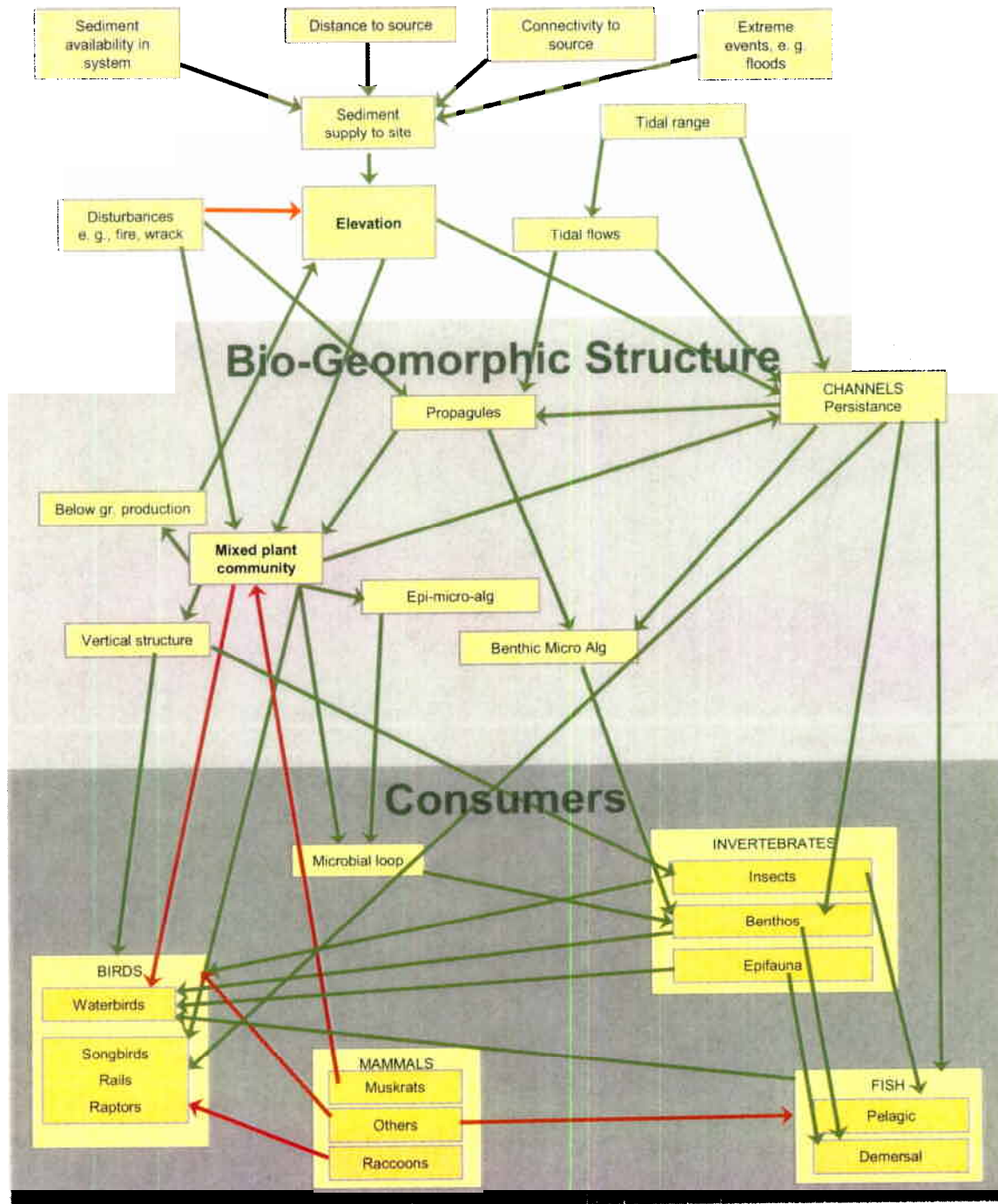
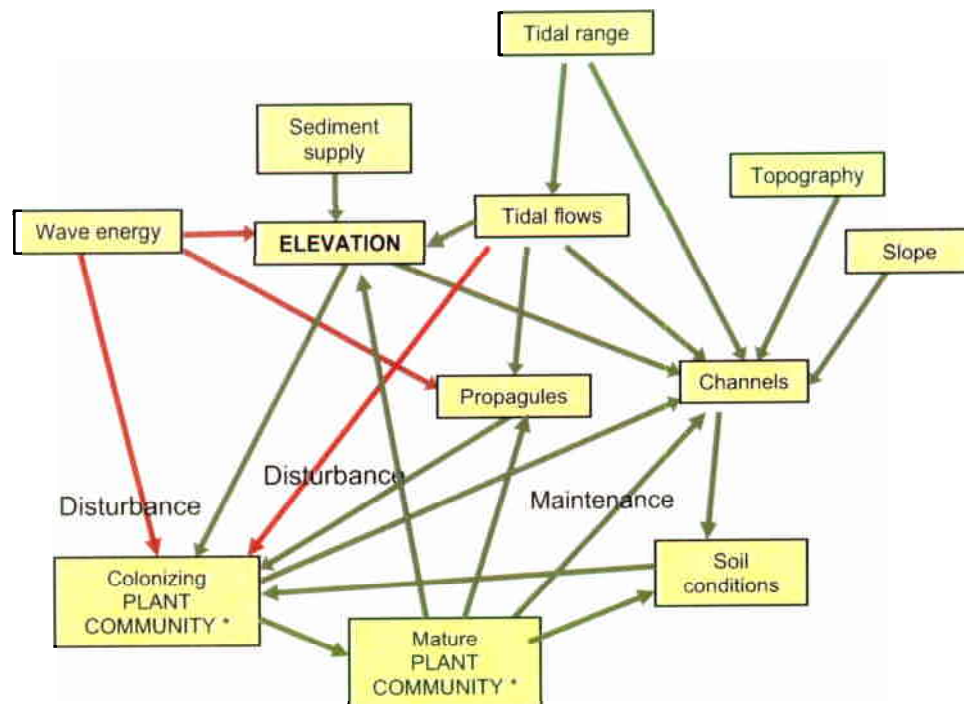


Figure 4 Vegetation colonization threshold conceptual model.



* Plant community composition is determined by a number of environmental factors. Once propagules are dispersed into a site, initial colonization is determined primarily through achieving species-specific establishment thresholds to a suite of abiotic factors, including soil conditions, elevation, and hydrology. Biotic interactions (such as competition) become more important in determining the mature plant community, which may also influence/modify the abiotic environment.

Figure 5 Study site with designation of intensive study sites in northern Lindsey Island and secondary study sites in Prospect Island West and Lindsey Island (BREACH I restoring and reference sites, respectively).

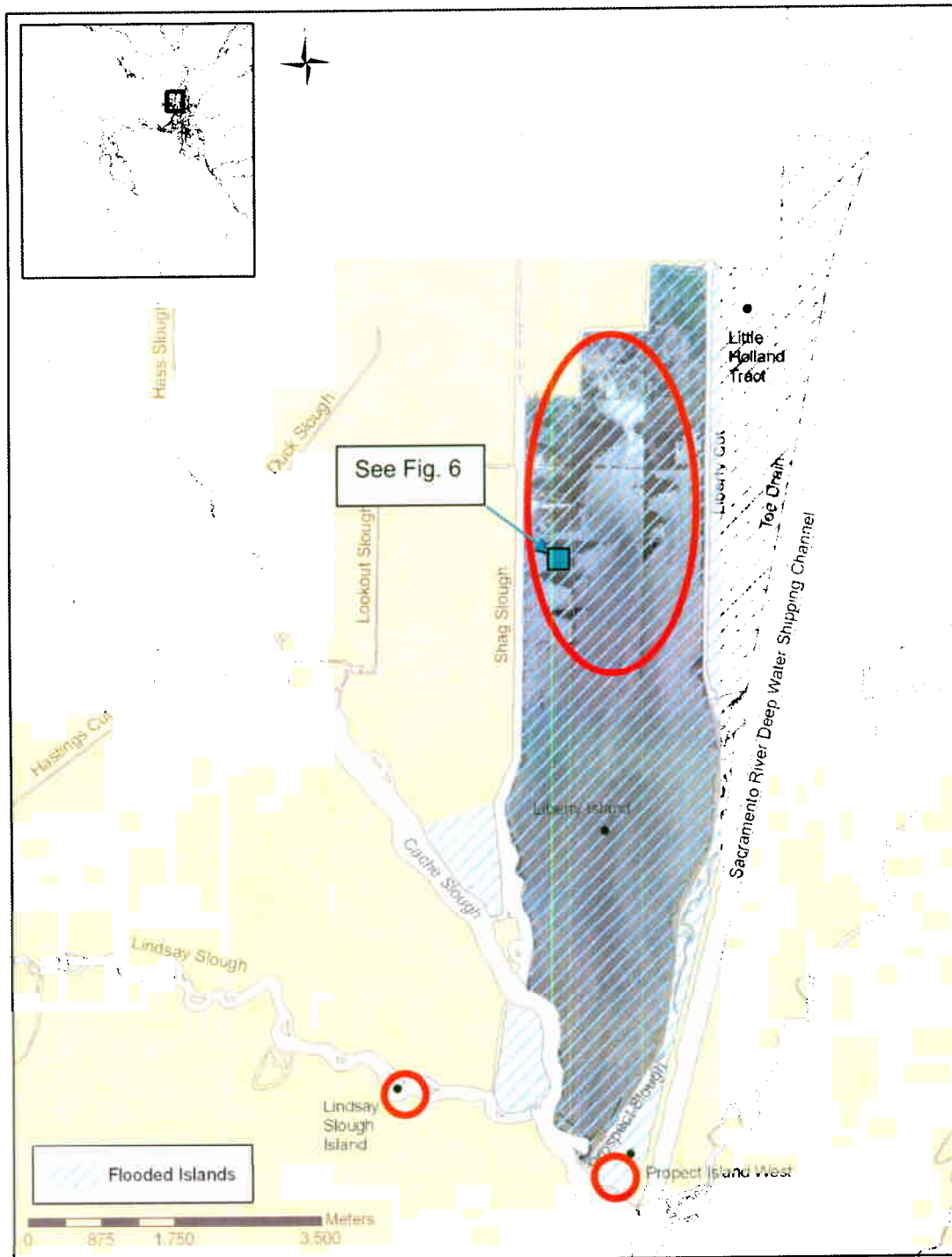
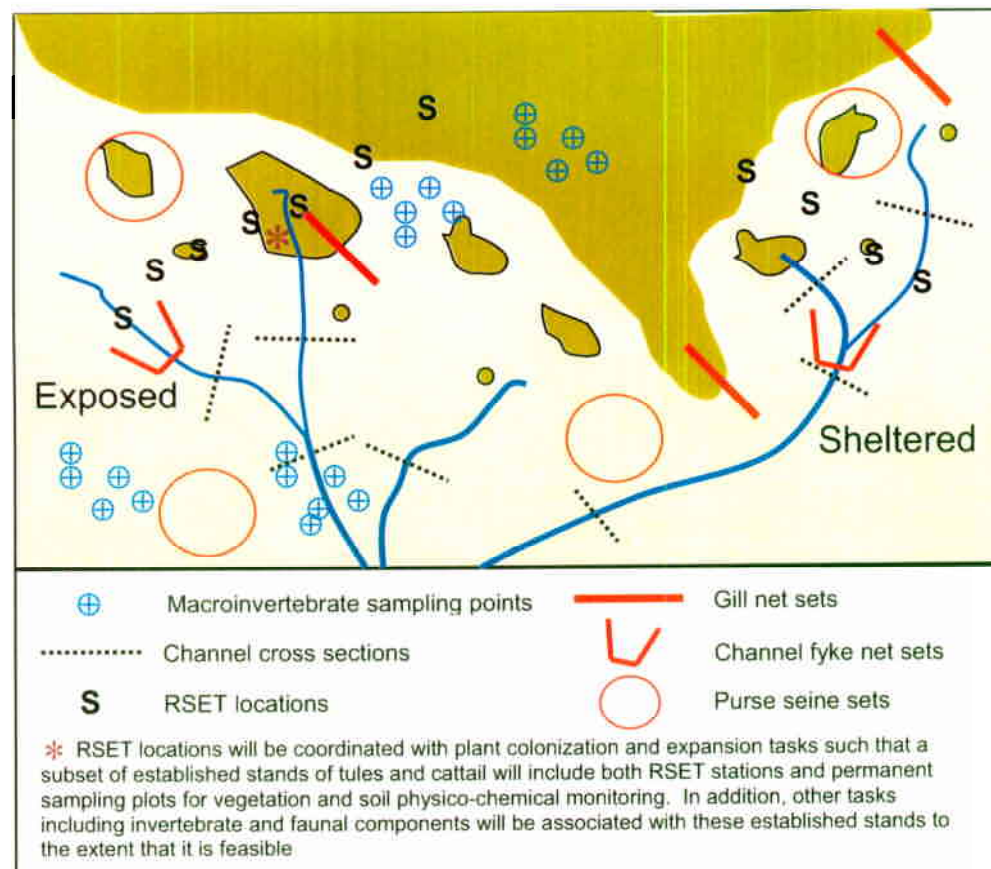


Figure 6 Schematic illustrating distribution of sampling types and units across vegetation (green) colonization threshold gradient.



Initial Selection Panel Review

Proposal Title

#0246: BREACH III: Evaluating and Predicting ‘Restoration Thresholds’ in Evolving Freshwater–Tidal Marshes

Funding:

Fund in part

Amount: \$1,500,000

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes
- Delta Smelt–related Projects

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

I view this study as leading to reduced uncertainty about the effects of CALFED investments in freshwater and tidal marshes. It will therefore improve our ability to focus future investments on high payoff projects and to improve project design generally.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

Some elements of the study seem more crucial than others. For

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Initial Selection Panel Review

example, it is not clear what conclusions can be reached about species (e.g., fish) that will spend only a limited amount of time in the restored habitat.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

This is a very expensive project. Given limited funding and the need to focus on the absolute core of CALFED's mission, this project should probably be pared down to the elements that give the greatest payoff w/r future CALFED investments in habitat restoration. With that proviso, the project should be funded.

Selection Panel (Discussion) Review

fund this amount: \$1,500,000

note:

fund in part

This project proposes work attempting to increase understanding of wetland development post-restoration. What are the relationships or causality after breaching a levee and restoring marshes in the North Delta? This research proposes a potentially strategic development of BREACH series, given a shifting focus to the North Delta as having highest restoration potential because of adequate rates of sediment delivery. The proposal includes many cooperating institutions and researchers. The ultimate goal is a predictive or deterministic model for wetland functions.

This work would be very relevant to wetland restoration, an important component of the CALFED program, and could help get the best payoff for money invested given the high cost of future wetland restoration.

Chief strengths of the proposal are in the modeling aspects. The geomorphology (sediment accumulation) and vegetation

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Initial Selection Panel Review

growth aspects of the project were particularly strong as well. The Panel agreed with the technical synthesis panel in questioning fish and avian studies - two years of field collection may not capture trends within larger variability. Also, macro-invertebrate and food web studies were not clearly linked to modeling.

A CALFED ERP grant for monitoring exists at Liberty Island and researchers should be encouraged to explore use of that data. This is one of the larger studies in terms of budget. The Panel recommended funding at a reduced level, particularly cutting or severely reducing Tasks 5,6,7,8.

Panel Ranking: Fund with modifications.

Technical Synthesis Panel Review

Proposal Title

#0246: BREACH III: Evaluating and Predicting ‘Restoration Thresholds’ in Evolving Freshwater–Tidal Marshes

Final Panel Rating
superior

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Previous work has shown how important this type of information can be. If there is going to be continued restoration of tidal wetlands then there must be a data base available so that each attempt does not learn the same lessons again. While this model will be based on just one site, the model will provide a starting point. None of the techniques are new or risky so all parts of the proposed study can be done. The fact that this continues on work previously done by this group of scientists is good reason to expect that the work can be done within the time frame proposed. The need for coordination is obvious, but this is not unusual for this type of study. This is a broad-based consortium of individuals from a variety of agencies and universities and represents an ideal approach to research.

Additional Comments:

There seems to be good coordination among PIs and with previous work. There is a problem when a model is based on just one site, but multiple sites would be very difficult. Previous work by this group has identified this area as one where the critical thresholds for marsh establishment has been reached. My judgement is that the basic physical setting

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Technical Synthesis Panel Review

necessary for marsh establishment will be clear at the end of the study using SETS and other direct measures of the physical & chemical characteristics of sites. The inherent variability of the faunal component will render these data more difficult to model in a meaningful way, although I am sure they will add to what is known regarding the fauna present during different stages of marsh development. The only glaring and potential problem is with respect to the biogeochemistry. Sulfide levels greatly influence vascular plant establishment and growth. These levels can change greatly as the soil system matures. Sulfide levels are greatly influenced by soil porosity, iron & manganese concentrations and evapotranspiration. The soils component as it relates to sulfide levels is a potential serious problem, not solved by one collection. This component should be enhanced.

Previous work has shown how important this type of information can be. If there is going to be continued restoration of tidal wetlands then there must be a data base available so that each attempt does not learn the same lessons again. While this model will be based on just one site, the model will provide a starting point. None of the techniques are new or risky so all parts of the proposed study can be done. The fact that this continues on work previously done by this group of scientists is good reason to expect that the work can be done within the time frame proposed. The need for coordination is obvious, but this is not unusual for this type of study. This is a broad-based consortium of individuals from a variety of agencies and universities and represents an ideal approach to research.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The applicants' previous work has justified the current project. The approach seems likely to succeed; none of the techniques used are new or exceptionally risky. The project team is outstanding, well-integrated, and has experience in this ecosystem. Therefore, the panel believed that project

Technical Synthesis Panel Review

implementation and success were highly likely.

All data generated by this proposal, and the subsequent model were viewed as essential to future restoration planning in the Bay-Delta ecosystem. The applicants may have overpromised a bit on the products they will deliver. For example, data from faunal surveys will be informative but probably will not be readily added to modeling efforts because of inherent stochasticity in these data and the short time-frame of the proposed studies (2-years). The overenthusiasm of the applicants does not detract from the benefits that WILL be derived from these intensive studies.

The major concern is that the applicants' will develop a model that is based on data from only one site. There was concern among the reviewers that this would limit the transferability of results from the model to other areas in the ecosystem. However, the depth of their data gathering efforts cannot easily be replicated on more than one site. The proposed research will establish a solid foundation for future validation of the models in other areas of the ecosystem. The intensive sampling will generate excellent results and this well-qualified project team is likely to use these data to refine their model into an excellent research and planning tool.

Rating: Superior

Technical Review #1

proposal title: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater-Tidal Marshes

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals, objectives and hypotheses of this project are stated clearly and in adequate detail. The overall goal, to use observation, experimentation, and numerical modelling based on investigation of three restoring freshwater tidal wetland sites in the Cache Slough watershed of the North Delta to predict the thresholds and trajectories of incipient ecosystem evolution is the holy grail of estuarine restoration science. The scope of the project is impressively comprehensive, including hydrology, geomorphology and sedimentology, primary producers and consumers, and secondary consumers (including invertebrates, fish and birds), with a special inclusion of species of concern (especially fish). Models are developed to simulate processes both at the habitat and landscape scales. The four primary sets of hypotheses are well designed, and the rationale and objectives of the ten outlined research tasks required to test these hypotheses provide a clear and coherent outline for accomplishing the projects admirable ambitions.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

#0246: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evol...

Technical Review #1

Comments	Unfortunately the six figures referenced in the proposal were missing, including a depiction of the Breach conceptual models. However, the models are well developed in the text. Fortunately I was familiar with the study area through figures and maps provided in another CALFED project that I have reviewed. The lengthy and detailed text cites many recent, excellent studies which provide a strong and credible base for the proposed work. The project builds upon two previous CALFED projects of the study area involving many (if not all) of Breach III project participants. The project will collaborate with and compliment other proposed CALFED studie(s) of the study area currently in review. The study described would be to my knowlege the most comprehensive investigation of the processes that drive tidal wetland ecosystem restoration undertaken to date. I agree with the project authors that a research project that develops and synthesized process based models of ecosystem evolution are appropriate and justified at this point in the history of North Delta restoration implementation, monitoring and research efforts.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The previous work of the project team in the study area through Breach I and II provides them with the necessary site specific knowledge and experience to develop a feasible approach to Breach III, especially
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Technical Review #1

	with respect to the selection of study sites and the sampling effort required to optimize statistical power. The explicit interest in describing thresholds of ecosystem development by synthesizing hydrodynamic, geomorphic and ecologic models is novel and exciting in the realm of tidal wetland restoration science. The methodologies used are sophisticated and state of the art, again optimizing the likelihood of producing the necessary data and information to achieve project goals. The models will produce extremely valuable guidelines for use by restoration managers in the selection and design of tidal wetland restoration projects that are likely to achieve a high degree of success.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The documentation for this project is clear, well organized, detailed and impressively referenced. The scope of the project required many pages of text, but oddly includes no supporting data or figures from the Breach I and II studies, although some project-specific data is presumably included in a number of relevant citations. A brief summary of the status of the outcomes of Breach I and II would have been welcome. Nonetheless, the experience and prior publications of the project team indicate a strong likelihood for project success. The authors are clearly able not only to conceive of a project of great conceptual strength but also to develop the concepts adequately with data and information gathered at both the habitat and landscape scale, as outlined. The inclusion of Figure 6 would have been helpful in grasping the full scope of the sampling effort.
Rating	very good

Technical Review #1

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The study includes a comprehensive variable set and an appropriate overall sampling design which includes both mensurative and manipulative experiments. Data collection is specifically designed to provide input for the conceptual and numerical modelling outlined. The modelling output will be synthesized to produce a quantitative model of tidal marsh ecosystem evolution. The development of individual quantitative models and their synthesis, as outlined in this proposal, are the highest and best use of any field monitoring and evaluation effort.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The scientific products of this research project will be of great value not only to restoration scientists but also to restoration practitioners and managers. The presentation of data and interpretation of results on the BREACH III website will also be extremely valuable to these groups. In addition, the project will commit resources to public outreach and education about project activities and outcomes through interactive web material. I suggest that a graduate student could investigate the tools now being developed by the "e-learning" community for use on the project web-site. The project will provide many fascinating results for
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Technical Review #1

	interpretation. The dissemination of project results through the open-access e-journal San Francisco Estuary and Watershed Science is also a unique opportunity available to projects such as this that focus on the Bay ecosystem.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The team is a strong cross-disciplinary group that provides the core expertise needed to achieve project goals with a high degree of success. All of the Ph.D. level team members have very strong publication and project management records. A number of team members have collaborated in the past.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Given the size of the project, the number of organizations/institutions involved, and the scope and length of the project, and the commitment to support the education of 5 graduate students during the three year course of the project, the budget is certainly reasonable. In fact, I am gratified to see a request for funds that seem adequate to support a truly integrated and comprehensive analysis of tidal wetland restoration/evolution, trophic structure and energy
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Technical Review #1

	flow. The indirect cost rate also seems very reasonable. The majority of the funds requested will directly support project staff and activities.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	My summary rating is logically based on the individual ratings above. The proposed project has great promise and merit, and will in my view raise our understanding of tidal wetland restoration processes to a new level of conceptual integration and completeness. The project will provide a strong model to build upon in other biogeographic regions where significant wetland loss or alteration has occurred, including the Gulf of Mexico and the Gulf of Maine.
Rating	excellent

Technical Review #2

proposal title: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater-Tidal Marshes

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goal of this project is to refine and parameterize predictive models of marsh development, and organismal response to marsh development, in flooded wetlands. The goal of understanding, at a level where prediction is possible, the forces and thresholds that drive flooded island restoration to tidal marsh are clearly stated and internally consistent. Similarly, the goals of understanding "assembly rules" for the resulting biotic communities are clearly stated and internally consistent. Each of these goals is very timely and very important as CALFED has, and will continue to dedicate resources to reestablishing functioning tidal marshes on flooded islands in the Sacramento-San-Joaquin "delta".
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The authors state a clear goal and propose intensive research in support of obtaining the stated goals. The conceptual model is that flooded islands either remain flooded or begin a deterministic process of succession
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Technical Review #2

based upon whether sedimentation rates surpass a certain (to be determined) threshold that allows establishment of marsh vegetation. Once the marsh plants begin growing, the hypothesis continues, the marsh will follow a systematic path of "community" assemblage that will lead to a "restored" marsh. The authors have completed two other studies (Breach I and II) that revealed a remarkable convergence to a "final" state among marshes undergoing succession. This observation supports their view that some threshold(s), early in the succession process, dictate(s) whether tidal marshes will be restored on a given flooded island. This justifies studying geomorphological rates (sedimentation, erosion, wave action, etc.) that might produce the critical elevation-gain rate that determines whether the rest of the process will move forward.

The authors also propose to study the dynamics of plant succession and this too makes sense and should lend itself to modelling.

Finally, the proponents plan to measure/monitor nekton and avian "community" assembly patterns, and both groups' use of marshlands undergoing succession. The justification here is less clear. In addition, there are several methodological concerns raised at these levels (see below).

The hypothesis (H3) that increased vegetative structure influences the structure, abundance, and behavior of animal communities is trite. More meaningful hypotheses would address the specific differences in animal assemblages at different stages of marsh development and the mechanisms that drive these differences across habitats.

I have the following general problem with this proposal, the researchers plan to use their studies to develop and parameterize several models that will predict whether and how restored flooded islands will

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	succeed into freshwater marshes. The key word is "predict". A prediction is just a prediction, and we won't know how good the predictions of this model are until they are applied to other real world examples of flooded island restoration. The fact that these models are being refined and evaluated based on studies of ONE island suggests that they may not be very good at predicting results on OTHER islands. Obviously, models can be re-parameterized to match the conditions found on different islands. If different driving forces dominate in one area than in the one studied or if threshold rates for DIFFERENT phenomena are met/not met on the different islands, we learn something by applying the model and seeing that its predictions are incorrect (this leads to a better model); but we are STILL not able to predict what the authors say they will be able to predict.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The authors plan to use a "space-for-time" substitution, wherein different habitats on Liberty island are deemed representative of different temporal "stages" of marsh development, for their analyses. But, implicit in the conceptual model is that marsh development from flooded islands is a "self organizing" process governed by critical thresholds. Thus, there is, by definition, a lot of spatial and temporal dependence (e.g. what happens in one part of the island affects adjoining regions). The spatial extent of this interdependence may change as different processes are
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considered (e.g. geomorphological processes at a particular point may influence processes at neighboring points over shorter spatial scales than, say, the spatial dependence of vegetative development processes). The problem remains that the authors are studying different processes over the same spatial extent and this may not produce reliable results or interpretations for some of the patterns they are documenting.

In addition, as the study turns from geomorphological processes to vegetative succession to fish assemblages to avian assemblages, the deterministic nature of phenomena is likely to decrease. I expect greater stochasticity to contribute to the latter variables. Thus, the temporal scale (duration) of the proposal may be appropriate for some of what the applicants hope to parameterize but not for other parts of their succession models. In particular, nekton and avian assemblage and ecological patterns are very likely to reflect specific conditions on Liberty island in the years (two) that these populations are studied. For example, whether or not the group catches Delta smelt, how many they catch, and where they catch them will probably be determined, in large part, by forces operating outside of the study region. The authors identify this phenomenon/problem (H3, p.11 of the proposal), but do not describe how they will study the strength of such spatially and temporally-dependent effects.

These issues of spatial and temporal autocorrelation are likely to confound the some of this project's results and make

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their usefullness in model parameterization problematic.

That said, I believe the project will contribute substantially to our knowledge-base. I am reminded of the seminal Hubbard Brook experiments that documented patterns of hydrological and biogeochemical response to terrestrial succession. Those studies made invaluable contributions to our understanding of forest ecology and riparian ecosystem processes. The information was hugely valuable to decision-makers in that case. Similarly, the detailed studies proposed here are likely to make seminal contributions to our understanding of geomorphological processes and biotic responses to those processes. However, as in the Hubbard Brook studies, the results of these studies may not be directly translateable to other "similar" systems.

Some of the fish sampling techniques described by the authors are almost certain to produce high mortality among specimens and these are likely to be (by design) T species.

The proposed isotope studies will not necessarily reveal the food web dynamics of the flooded island since organisms (particularly fish and birds) may move on an off the island in search of food. The information should be interesting and help generate testable hypotheses re: fish and large macroinvertebrate resource use; but, it will be difficult to say much for certain about the ecological/behavioral patterns that produce these results. For example, delta smetl are known to spawn and

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	<p>rear in several different places in the delta at different times during the spring. Two delta smelt may have the same isotope signature even though they arrive at those integrative isotope ratios through different migratory pathways. Also, inferences about food-web dynamics may be particularly sensitive to conditions (e.g. Delta outflow) in particular years --this is especially true for upper-level consumers.</p> <p>The fish sampling is intensive. The authors do not address how they will deal with different sampling efficiencies of the different gears that they will use in different marsh habitats. They may have real difficulty comparing density and richness measures across habitats because of the habitat*gear interaction.</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The approach is technically feasible and very well-documented (the applicants are to be congratulated for providing a reasonable amount of detail describing their methodologies). This group of applicants is highly likely to produce meaningful results.</p> <p>Project scale: The temporal and spatial resolution (grain size/sampling intensity) are not completely appropriate. The temporal and spatial extent of the project are, in some ways, too limited (see above). I think the authors will make good progress on</p>
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	<p>parameterizing geomorphological rates and thresholds and reasonable progress on understanding the vegetative response to the geomorphic processes. They may gain insight into the feedback between vegetative development and subsequent geomorphic development (channel formation). However, the temporal and spatial extent are too narrow to truly understand the response of animals to these processes. The project will likely produce a predictive model for geomorphic and vegetative responses but will not validate that model so that decision-makers will know how well it works.</p> <p>The applicants seem to have chosen to improve our DEPTH of knowledge by limiting the spatial extent of their study. That may be a valid compromise as they might not be able to complete such detailed, intensive studies in additional locations than they have planned.</p>
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	<p>Applicants are monitoring the succession dynamics of a flooded island. They have experience studying similar sites in the vicinity. There is some mention made of the ability/desire/intention to compare findings from Liberty Island with those from other areas in the same Estuary. More should be made of these comparisons as they would expand the spatial extent of the study and the predictive capacity of the resulting model.</p>
Rating	not applicable

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Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	<p>I believe this is a valuable project. When such a qualified and well-integrated team proposes to gather so much data to understand such a critical restoration phenomena, there can be little doubt that the products will be a valuable contribution to the overall management program.</p> <p>The principle results of this project will be conceptual and predictive numerical models. Above, I have addressed reasons why some parts of this study will not produce well-refined models. I believe the studies of geomorphic rates and thresholds and subsequent vegetative response will, nevertheless, be extremely valuable. Information gathered on aquatic and terrestrial animal responses will serve as a great foundation for studies that must continue for both a) longer periods and b) over a larger spatial extent. I believe these components of the study should be funded but only with the understanding that they must continue for longer periods over greater spatial extents or the results won't mean much.</p>
Rating	very good

Additional Comments

Comments	<p>I was surprised that the proposal did not address the concerns above. For example, the authors clearly address that they are aware of the limitations imposed by spatial and temporal scale (e.g. H3b, p11.) but they do not clearly identify how these limitations will limit or change their analyses and products.</p> <p>Also, I was surprised at the many typological and</p>
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	grammatical errors in the proposal. I could not find the figures referenced in the proposal. I realize that this error could have occurred for a number of reasons, some are outside the control of the applicant. But, the applicant could have checked to see how their proposal would appear to reviewers and they did not correct this problem. Finally, Despite the fact that this is a very detailed and complicated proposal, I don't think it required 50+ pages. The methods were detailed (this was welcome) and there were many of them -- the applicants could have placed more attention on succinct description of the goals and justification for their project.
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The PI's have outstanding track records. Additionally, they have an excellent record of working together, in an integrated fashion, within this ecosystem. There is no doubt that this is the best team to conduct this very necessary research.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This is a very complicated project and I am not capable of fairly evaluating the budget request for all aspects of the proposal. On the whole, the \$2+ million price tag seems reasonable for the number, quality, and variety of investigators involved and the intensity and diversity of the studies proposed.
Rating	very good

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Overall

Provide a brief explanation of your summary rating.

Comments	<p>The proposal is important. It will provide very useful information. The data collected can only be interpreted within the limits of the methodology and scale with which it was collected. The proposal suffers from lack of detail about how the site-specific conditions of liberty island are impacted by this flooded island's landscape context -- for example, the vegetative succession patterns here are no doubt influenced by the available plants in the surrounding matrix. Similarly, the study's duration (two years) is short for some of the phenomena under study -- avian and fish assemblages and their use of marsh habitats are likely to be heavily influenced by conditions in the particular years that the study is conducted. These shortfalls are at least partially mitigated by the intensity of data collection planned and the excellent integration of the different studies.</p> <p>The research team is stellar. I do NOT think they can deliver everything they appear to have promised (e.g. models will be ready for testing, not necessarily for use in restoration planning), BUT, what they do deliver is likely to be substantial and highly valuable.</p>
Rating	very good

Technical Review #3

proposal title: BREACH III: Evaluating and Predicting 'Restoration Thresholds' in Evolving Freshwater-Tidal Marshes

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The premise of the proposed work, which is based upon the authors previous research (BREACH I and BREACH II), is that the initial colonization by vegetation is an important threshold during the process of wetland restoration. Following from this premise, the overall goal of the proposed work is to obtain a predictive understanding about the biotic and abiotic controls on this threshold as well as the physical, biological and ecological changes that take place during the process of vegetation establishment. The clearly stated objectives and hypotheses rest upon the assumption that vegetation colonization causes an abrupt state change. The authors lay out 4 objectives for their work, wherein the aim to (1) understand the influences on colonization, (2) compare pathways of colonization, (3) understand environmental responses to colonization and (4) develop a predictive model aimed at application to future restoration activities. These objectives remain consistent throughout the proposal, but perhaps some additional information/experimentation in the proposed work (detailed below) might help to strengthen what is learned from objectives (1) and (2). The overarching hypothesis, which again draws on the authors' extensive previous work, is that colonization is a deterministic and predictable process. Subhypotheses suggest that (1) colonization is dependent on local and landscape scale hydrogeomorphology, (2) including</p>
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	<p>channel evolution, that (3) landscape structure dictates local animal abundance and (4) that stochastic events (storms/floods) provide important structuring forces. These hypotheses remain consistent throughout and all components of the proposed work are designed to substantiate the hypotheses. Is the idea timely and important?</p> <p>Yes. The previous BREACH projects have provided broad-scale information on the process of restoration in these tidal freshwater marshes. The work proposed herein outlines a very detailed and specific set of data acquisition and experimentation that should guide a more predictive model of vegetation establishment. At this point, we still know relatively little about the factors that may promote vegetation establishment and this work will help to guide future restoration efforts.</p>
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>Is the study justified relative to existing knowledge?</p> <p>Little work has been done on tidal freshwater restoration in the northern reaches of the Delta. This is particularly true of sites at the very initial stages of restoration. Therefore this study is timely and justified.</p> <p>Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work?</p> <p>The figures are missing from the proposal that I obtained on the CalFed website, which hinders my</p>
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	<p>ability to comment on the conceptual model. As described in the text of the proposal, this model appears to put forward an explanation of the process of plant colonization, stressing important feedbacks between physical and biological processes. Of particular importance to this proposal are the ideas that local hydrogeomorphology (particularly sediment supply) control plant establishment and that plant establishment, once it occurs, controls not only biological interactions but also local physical processes (particularly channel development). I believe that this model is based upon the premise that plant colonization is the critical factor in restoration outcome, and that there are a complex array of physical (and biological?) drivers that dictate the process of restoration. My rating of very good, below, is based on the fact that I don't have access to the figures and therefore cannot adequately evaluate the conceptual model.</p> <p>Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?</p> <p>Yes. This research project is justified based upon prior studies done by the current team and our existing knowledge base of the process of restoration in this reach of the Delta.</p>
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Is the approach well designed and appropriate for meeting the objectives of the project?
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For the most part, the approach is very well designed and appropriate. The geomorphological data to be collected is detailed, substantial and should provide strong supporting information for the predictive capabilities of the modeling exercise. Likewise, the assessment of abundance and diversity of higher trophic levels is also adequate. I believe that this is a very strong proposal, however, tasks related to the establishment of vegetation could be expanded to provide more detailed information on the controls and a more powerful predictive model.

There will be a solid base of understanding the physico-chemical controls on vegetation establishment, but not so for the biotic controls. Competition between the 3 dominant species is the only biological control to be assessed (Task 4). In particular (and perhaps this will be done but is not stated explicitly), in Task 2 (landscape structure and change) I believe that distances between established meadows and newly emergent patches should be evaluated. There may be an Allee effect controlling recruitment during the early stages of colonization when patches are few and far between. Pollen limitation should be assessed as a potential controlling factor for new recruitment. Likewise, vegetative spread of existing clones is another important means for the spread of plants onto previously unvegetated mudflats. What are the important factors controlling this spread? Nutrient availability, local anoxia, salinity, etc... could be investigated experimentally for both the establishment of new seedlings and clonal expansion. This work would be much stronger if a better mechanistic understanding of plant

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colonization (experimentally based) was included.

It is unclear to me where the information for macrophyte module of the ecological model is derived. Macrophyte productivity is based on hydrological information - how does this work? Where does the calculation of NPP for each plant community come from? Will local biomass within different patches (based on stratification of age, size, elevation, etc...) measured in this study? It appears that biomass will be calculated based on biomass, maximum growth rate (from where?) and "a limiting function" that appears to be derived from a literature value? This isn't adequately explained. The primary objective of this work is to understand the dynamics surrounding vascular plant colonization; however it appears that the majority of the information to be used in the predictive ecological model is not derived from local, explicitly measured conditions. The authors cite Allison (1996) who described the two important mechanisms for vegetation establishment: seedling establishment and clonal growth. It isn't clear to me that a clear understanding of these two factors will be obtained from the experiments described in task 4 (plant colonization dynamics). While I am not qualified to comment on the explicit mechanisms of the proposed model, it does seem important to include empiric data from the local environment.

A more minor problem with the methods relates to the evaluation of food web dynamics. Another area that I believe may need some refinement or further investigation is in the assessment of foodweb structure. Because of the increasing ease and decreasing cost of

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natural abundance stable isotope analysis, this technique is often used in food web studies. However, these studies are often labor intensive and time consuming and the results are often difficult to interpret conclusively. It is proposed herein to evaluate the diet of "dominant members of the fauna utilizing different habitat elements" by selecting species representative of each habitat type. It is likely that this will provide results suggesting that each species forages within the appropriate habitat. It may be more fruitful to select species that are ubiquitous across habitats, and determine whether foraging behavior and trophic status shift upon plant colonization.

A final area that could be addressed, (this may be planned for future efforts?) is the validation of the predictive model to be developed. It would be interesting to see how the predictive model holds up at other sites within this region.

Is the approach feasible?

As outlined I believe that the work is entirely feasible within the budgetary and time constraints proposed.

Are results likely to add to the base of knowledge?

As proposed, I believe that the results will add to the base of knowledge regarding restoration of tidal freshwater marshes within this reach of the Delta.

Is the project likely to generate novel information, methodology, or approaches?

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	<p>The information generated regarding the transition between vegetated and unvegetated restoration sites, and the potential physical and biological factors that may control this process are novel and valuable outcomes.</p> <p>Will the information ultimately be useful to decision makers?</p> <p>The information on plant establishment should provide direct information for decision makers and managers. This is particularly true for the physical controls on colonization. Managers should be able to create the "correct" physical conditions for successful plant colonization based on the outcomes of the predictive models proposed herein. To date, we don't have a solid predictive model for plant establishment and this work should provide a more solid understanding of the controls on plant establishment. This work will provide more information and more detail on the very specific processes that occur as plants recolonize newly breached islands within the delta.</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Yes, it appears that the project is fully documented and technically feasible. The amount of work appears to be reasonable for the time and budget constraints and should be successful. The scale of the project is consistent with the objectives. Because of the large scale and interdisciplinary nature of the work, a great deal of project management and integration is
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	required. It appears that this has been thought through quite carefully and that the level of communication and data-sharing between the individual components should be adequate to achieve the project goals. This will not be an easy task given the nature of the project, but should be feasible given the assembled team of scientists.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	While I don't believe that this work was explicitly designed to be a "monitoring" effort, the data produced should provide the ability to evaluate the success of restoration in the Liberty Island region and will dictate appropriate metrics for future monitoring within similar areas. The models generated as a result of this effort will be shared with other groups and managers working in this area. The models should provide a useful future tool for the more consistent management of restoration activities on flooded islands. There are plans to create a project website that will increase the dissemination of information within the community. Likewise, the authors plan to bring in outside reviewers along the way to help to guide and provide incremental peer review as the project proceeds.
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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Comments	<p>The predictive model to be generated by this work will be of great value to managers within this region. The authors further propose to integrate their information gathering attempts with all others who work in this region, which is eminently important to the success of all CBDA efforts. If this and other proposed work in this area are concurrently funded, integration of data acquisition, interpretation and sharing should be maximized. Likewise, methodologies should be standardized across projects such that all data can be coalesced into a single database. The maintenance of adequate metadata for all proposed projects is very important and should be emphasized.</p> <p>The interpretable outcomes of this project include a better understanding of the processes that control macrophyte establishment on newly flooded islands in the Delta. The biotic controls on plant establishment could be improved, as discussed above.</p>
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	<p>This is a very well qualified team with a solid track record in ecological research and restoration. The lead PI, Si Simenstad, has many years of experience with similar efforts and is highly qualified to carry out a project of this size. New investigators have been brought into this phase of the BREACH studies to augment the modeling needs, which adds to the strength of the team. Simenstad serves on many of the teams, adding consistently across the different efforts. The</p>
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	collective experience of this team makes them duly qualified to carry out the proposed work. Through local resources and connections within the field area, as well as infrastructure at each of the home institutions, this team has the support necessary to complete the proposed work.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Yes, the budget seems reasonable. There is not a lot of detail in the budget justification to explain where some of the expenses are going, but the total amount seems reasonable for a project of this size and level of detail.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	My review may be somewhat limited because I do not have access to the figures associated with the proposal. However, based upon what was available to me, I believe that the proposed work is of high quality and if successful (which I believe it will be) should provide valuable information on the process of restoration in the Delta. There is additional information on the vegetation which I believe could be added to strengthen the proposal. However, this appears to be a solid proposal with a high chance of success.
Rating	excellent

